

Magazine

My word

In defence of passion Andrew Murray

Graduate students and post-docs often ask “What do I need to do to succeed?” One popular answer is to boost your career by networking feverishly, finding powerful mentors, learning to pinpoint hot areas, and publishing in sexy journals. This approach has two problems: it makes succeeding in biology sound like learning to sell used cars, and it relies heavily on prognostication, a skill that even politicians have yet to master.

I want to discuss a more old-fashioned solution: do work that you feel excited and passionate about. The first step is to find a problem that is hard to solve, located in an unpopular area, and interesting enough to push you out of bed every morning. If your problem is hard, you'll have to endure more failure in your quest. Because failure hurts, most of us learn from it faster than we do from success. In addition, succeeding after a long struggle gives you confidence that you can tackle other difficult problems. Working in an unpopular area gives you the time to fail and learn without worrying about an enormous factory lab overtaking you in the night. Finally, your problem must generate the excitement and passion that will keep you going through thick and thin.

How do you find the perfect problem? Start by consulting less fashionable sources. Try leafing through solid but unflashy journals for papers that identify new puzzles, and talking to older scientists about interesting problems that have been waiting for fresh young blood. Information from more than a century of interesting experiments on an enormous diversity of creatures is lodged in the cerebral crinkles of

people whose careers began before the dominance of molecular biology and model organisms.

If you're interested in cell biology, consider reading E.B. Wilson's classic book *The Cell in Development and Heredity*. Any question that Wilson asked in 1925 and is still unanswered is likely to identify an interesting problem. For example, what is a centrosome made of, how does it duplicate, and how does it position itself within a cell?

A good problem should be hard to solve and located in an unpopular area

Another way of finding good problems is to consult yourself. List the questions that you think we have to answer in order to reach an integrated understanding of how cells, organisms, and populations work. I bet half the questions on your list are unanswered in principle, let alone in detail. For example, although we've made great strides in understanding how transitions in the cell cycle are regulated, we know next to nothing about the factors that determine how cells control the rate at which their mass increases (the real meaning of cell growth).

Although your problem should be hard, it has to be soluble. There are three ways that insoluble problems become soluble. The first is by the sort of mental leap that occurs when someone rephrases a question in a way that allows existing techniques and resources to answer it. The more you know about your field and biology in general, the better your chance of asking your favorite question in a new way. The second is through the use of new techniques. The more techniques you can use and understand in detail, the more likely you are to invent new ones. The third way in which intractable puzzles often seem to be solved is

through a chance observation, although most of these discoveries are less lucky than they seem. Near the end of my graduate work, I failed to pursue a strange finding that wrecked a measurement that I was trying to make, choosing simply to get the data I badly needed by taking a different approach. Later, Dan Gottschling's better-prepared and more flexible mind took a similar observation and made it into the beautiful story of how telomeres silence nearby genes.

Once you've found a problem you love, you need to find a boss who shares your curiosity or is benign enough to welcome a well-meaning eccentric. You'll hone your communication skills by convincing a mentor that it would be fascinating to know why echinoderms have five-fold radial symmetry. This exercise is also valuable preparation for giving talks to similarly unenlightened audiences after you've solved the puzzle.

Finally, you can embark on your project. Of course, you need to do well-designed, meticulous experiments, be determined in the face of failure, and generous to your predecessors when you succeed. But even more importantly, you need to work openly and get as much criticism and advice from other scientists as you can. The beauty of working on an unpopular problem is that you won't need to worry about some dark prince or princess skewering you with your own unpublished results, and because you've picked a hard problem you'll be forced to seek advice and consolation on a regular basis.

I spent my graduate career unsuccessfully trying to discover why chromosomes care how long they are, published a paper last year that had data from my thesis in it, and am still trying to persuade my poor students and anyone else who is interested to help figure out the answer.

Address: Departments of Physiology and Biophysics, University of California, San Francisco, California, USA.